Hartshorne (H.)

CHOLERA:

FACTS AND CONCLUSIONS

AS TO ITS

NATURE, PREVENTION, AND TREATMENT.

BY

HENRY HARTSHORNE, A.M., M.D.,

MEMBER OF THE AMERICAN PHILOSOPHICAL SOCIETY; FELLOW OF THE COLLEGE OF PHYSICIANS OF PHILADELPHIA; PROFESSOR OF HYGIENE IN THE AUXILIARY FACULTY OF MEDICINE IN THE UNIVERSITY OF PENNSYLVANIA, ETC.

PHILADELPHIA:

J. B. LIPPINCOTT & CO.

1866.

CHOLERA:

FACTS AND CONCLUSIONS

AS TO ITS

NATURE, PREVENTION, AND TREATMENT.

BY

HENRY HARTSHORNE, A.M., M.D.,

MEMBER OF THE AMERICAN PHILOSOPHICAL SOCIETY; FELLOW OF THE COLLEGE OF PHYSICIANS OF PHILADELPHIA; PROFESSOR OF HYGIENE IN THE AUXILIARY FACULTY OF MEDICINE IN THE UNIVERSITY OF PENNSYLVANIA, ETC.

PHILADELPHIA:

J. B. LIPPINCOTT & CO.

1866.

ON CHOLERA.

SOME experience with Cholera, in this city in 1849, 1850, and 1854, and in the latter year at Columbia, Pa., and the perusal of most of what has been written upon the subject, have impressed me with opinions, some of which amount to strong convictions; the truth of which—if they *be* true—is so important as to make it justifiable, if not a duty, to give them publication at the present time.

Without claiming novelty for my views (some of which were published in a medical journal more than ten years ago*), my purpose is to endeavor to give the results of a careful analysis of the facts, and, if possible, an approach to a consistent theory, with direct practical applications. To this end, it will be best to attempt a brief digest of the subject, in a methodical form.

Principal Synonyms. — Epidemic, Spasmodic, Malignant, Algid, Asiatic, Indian Cholera; Cholera Asphyxia, Mort de Chien; in India, *Vishuchi* (vomiting and purging); at Bagdad, *Haouwa* (tornado).

---

* Philada. Med. Examiner, Aug. 1855; On Animal Decomposition as the Chief Promotive Cause of Cholera, etc.

DEFINITION.

An acute systemic epidemic disorder (endemic in parts of India), not contagious, but produced under certain local circumstances by an unknown specific cause, which appears to be in rare instances and to a very limited degree portable; the symptoms being, in most instances, a premonitory, painless, and mostly watery diarrhœa of variable duration, followed by vomiting, also watery, and increased diarrhœa, with weakness, coldness, intense thirst, difficulty of breathing, loss of voice, cold breath, cramps, disappearance of pulse, suppression of the action of the kidneys, restlessness, and a blue or livid and shrunken aspect of the skin; which symptoms may end in death in from ten minutes to forty-eight hours—in relief and rapid recovery within twelve hours or more—or, in a partial reaction and low fever (chiefly uræmic), the result of which is either death in a few days, or recovery in a week or two.

APPEARANCES AFTER DEATH.

Rigidity occurs *soon;* sometimes in less than an hour; generally within two hours. Startling *movements* of the corpse have been several times noticed; as of a patient, dead with cholera, slowly

lifting both hands over the chest and joining them; opening the eyes and rolling them downward, etc. *Increased heat* of the body, cold during the attack, has been sometimes observed after death. Internally, several of the great organs, the brain, spleen, and kidneys at least, are commonly gorged with blood. So are the *right* cavities of the heart; but the left side of the heart empty or with but little blood, and firmly contracted. The lungs are almost bloodless. The liver varies in appearance; but the gall-bladder is almost always *full of bile.* The urinary bladder is, constantly, greatly contracted. The stomach and intestinal canal are congested, and swollen; the late Prof. Horner observed the frequent throwing off of the "epithelial" lining of the canal; Böhm, of Germany, confirmed this; Drs. Parkes, Gull, and Lindsay assert it to be a *post-mortem* occurrence.* The intestinal glands are found considerably enlarged. The *blood* has been carefully examined by Drs. Garrod, Schmidt of Dorpat, and others.† Its water and salts transude into the alimentary canal, with some of the albumen and fibrin; also *the contents of the blood-cells transude into the serum.* The blood drawn from a vein during life is (as I have seen it) dark, thick, and tarry, scarcely capable of

* Edinburgh Med. and Surg. Journal, Jan. 1855.

† Brit. and For. Medico-Chirurg. Rev., July, 1854.

flowing. Schmidt found the amount of oxygen in the blood-corpuscles less than half the normal proportion. The blood is *acid* sometimes in cholera; the reverse of its natural reaction.

The *ganglia* of the "sympathetic" system have been often examined, and are frequently changed in appearance; congested, softened, altered in color; but no *special* change has been *shown* to belong to them in cholera.

DIAGNOSIS.

Common cholera morbus alone (*absurd* name, hybrid of Latin and Greek; as absurd also is cholera, from the Greek for bile, a cognomen for a disease in which the excreta are remarkable for the *absence* of bile), when severe, resembles epidemic cholera so much as to be easily mistaken for it. The *collapsed* stage of the one, preceding death, is almost identical in appearance with the collapse of the other. But cholera morbus is *caused* by some irritant of the stomach and bowels, and is clearly an affection of *those organs*, not a *toxæmia* or systemic disorder; it is sporadic, *not epidemic;* in it the discharges are always *bilious* at first, and mostly so to the last; collapse in any degree is *rare*, and death, under judicious treatment very uncommon. In all these things, it differs greatly from Asiatic cholera.

HISTORY.*

Putting aside some possible resemblance to this disease in descriptions of Aretus and one or two other ancient authors, probably the epidemic in France of 1545, "*trousse-galant*," came more near to it. The earliest distinct account of cholera was that given by Bontius, a Dutch physician of Batavia, 1629. Willis (1684), Morton (1692), and others, described epidemic fluxes and "dysenteries" in England in such terms as strongly to remind us of cholera; and so did Degner of Nymwegen, in the Netherlands (1736), and Morgagni in Italy, in 1733. Some British physicians (Greenhow, Aitken) now believe that cholera may have repeatedly visited England. It appears to me more probable, however, that this opinion is due to an overestimate of the resemblance between the autumnal cholera morbus of Great Britain (like our own) and the pestilential disease.

Certainly cholera must have existed in India for an indefinite time. From 1781–2 dates its extended

---

* In this sketch I am chiefly indebted to Dr. A. Brigham's Treatise on Cholera, Dr. E. H. Greenhow's and Dr. Gavin Milroy's papers in the Brit. and For. Medico-Chirurg. Review (April, 1856, and October, 1865), and Boudin's Traité de Géographie et de Statistique Médicales, tome ii.

prevalence, in a most destructive form; at Calcutta, in Madras, on the Coromandel coast, and in Ceylon.

In August, 1817, Jessore was the birth-place of the first great migratory epidemic. Shortly after, in Calcutta, 36,000 were attacked in three months. At many military stations, it was very severe. Roads were covered with dead and dying, unable to reach their homes. In November, the grand army of the Marquis of Hastings was devastated by it. Of 90,000 men, in twelve days 9000 had died. On marching the army across a river to dry and elevated ground, the commander was relieved of this otherwise invincible enemy.

In 1818, the Birman empire was invaded by cholera; and there and elsewhere in Asia, its ravages were fearful. In 1819, 150,000 died of it in the Presidency of Bombay. It also reached Mauritius, 20° S. latitude, three thousand miles from any place before visited by it. The Island of Bourbon was visited in 1820; as well as the Philippine Islands. In 1821, Borneo and Java were affected; and a large Persian army was repulsed by it from before Bagdad, without a battle. In 1822 its limits were much narrowed, and its destructiveness abated.

India almost escaped in 1823, but China was ravaged by it; and it extended northwestward,

in that year, to Orenbourg, on the Ural, near the borders of Europe and Asia. In 1826 it passed the great wall of China in its northward progress; but almost left Western Asia. It reappeared in Persia in 1829.

Orenbourg was revisited in that year, and the epidemic there lasted from August to near the end of February. This city had a population at that time of 11,000, of whom 6000 were soldiers. Those first affected had no communication whatever with any infected place.

1831 saw the cholera in the north of Europe, as far as Archangel, near the Arctic Ocean, more than 64° N. latitude. It reached Warsaw in April, during an insurrection, and was very fatal. Hungary suffered from May to September; losing 100,000 of its population. In June, St. Petersburg, and in September, Moscow, were reached by the pestilence. Berlin had it also for three months and a half, beginning in August. Mecca was attacked during the visitation of throngs of pilgrims, in May; of 50,000, as many as 20,000 are said to have perished. In this year, while Hungary was infected, the Austrians surrounded Vienna by a double *cordon militaire;* but in vain. The disease began there in August and continued for three months. The southern provinces of Austria and the Rhineland were exempt. Con-

stantinople was affected by it, but not with very great severity. The Turkish government, that year, maintained no quarantine. Cairo suffered dreadfully in 1830–31; and so did Smyrna.

Attacking Hamburg on the 11th of October, 1831, it was officially announced at Sunderland, England, October 26th. It had occurred in several cases in England months before. Three or four weeks later it appeared at Newcastle; and in December, at Haddington, a Scottish town on the Tyne.

Edinburgh and Glasgow first had cholera in January, 1832; London in February; Dublin and Paris in March. London then suffered but moderately; Paris terribly — especially in April and May; 20,000 deaths.

On the 8th of June, it first invaded our continent, at Quebec; and within a week, at Montreal. In the same month it was in New York and Albany. Philadelphia had its first cases in July. Between the 1st of July and the 18th of August, New York had reported 5337 cases, with 2068 deaths. That city lost 3513 in all.* From the 27th of July to August 18th, Philadelphia had 1610 cases, with 615 deaths. Boston and Baltimore were moderately affected in August.

---

* Dr. A. Clark, Lect. on Cholera. In 1834, New York lost 971; in 1849, 5071; in 1854, 2509.

Detroit, Buffalo, Elizabeth City in North Carolina, Wilmington and Newcastle, Delaware, Norfolk and Portsmouth, Virginia, and New Orleans were the principal of more than fifty towns in the United States reached by cholera in 1832. It had entered twelve different States before September.

Havana and Mexico were attacked in the spring and summer of 1833. The *City* of Mexico, notwithstanding its great elevation above the sea, did not escape.

Portugal was also first visited in that year; Spain but slightly until 1834. Northern Italy was affected in the autumn of the same year. In 1835, Alexandria and Malta; in 1836, Rome, Naples, Egypt, and Central America especially suffered. North Germany, South France, Rome, Naples, Sicily, Malta, Egypt, and Syria, in 1837. After that, cholera disappeared from Europe and America for nearly ten years. It still existed, in variable violence and extent, in India.

In 1847, it ravaged a Russian army west of the Caucasus; and in September returned to Moscow. In 1848, Turkey, Russia, Austria, Prussia, Belgium, Holland, Great Britain, and France (though not Paris) were successively attacked. Then the cholera showed its power to traverse the sea without human aid or agency, by attacking two emigrant ships, a thousand miles apart, one sixteen and the other twenty-seven days out from Havre,

*when no cholera was prevailing at that port.** The *cholera-cloud* itself also reached New Orleans about the same time, and progressed up the valley of the Mississippi. New York was not affected by the visit of the infected ship; the disease not occurring again there until May, 1849.

Paris was reached by it in February of that year, but suffered the worst in June. Lyons now had it for the first time. Tunis and Algiers were visited toward the end of the year.

In January, 1849, after Memphis, St. Louis, Missouri, was attacked. Chicago, Buffalo, and other towns on the lakes, in May. New York and Philadelphia in the same month. Baltimore had this year only a local epidemic, in July, in the Almshouse; the restriction of which to one side of the building was very remarkable. As in 1832, the mortality in Philadelphia was much less for the population than in New York: 1022 deaths occurred in our city; New York had a mortality 450 per cent. greater.† Canada was reached this time from the westward.

In 1848–9, the number of deaths from cholera in England and Wales was over fifty-four thousand (54,398); in 1832–3, nearly thirty-one thousand

---

* Report on Cholera in the United States, by Dr. James Wynne; and Dr. Gavin Milroy, Brit. and For. Medico-Chirurg. Review, Oct. 1865, p. 444.

† Dr. J. H. Griscom, Medical Record, March 15th, 1866, p. 35.

(30,924). In London,* probably owing to greater attention to sanitary means, the mortality was two-fifths less the second time than the first. Some parts of southern Rhineland were visited in 1849; especially the filthy City of Cologne.

Cholera lingered in various places almost sporadically, in Europe and America, from 1850 to 1854. Canada and the far West (Indiana also had cases every year) suffered the most, in this way, on our continent. In the West, emigrants' camps and military stations seemed especially to furnish its required local conditions.

In 1853, Persia had it severely; also some parts of Northern, Central, and Southern Europe (Copenhagen, Hamburg, Berlin, Piedmont, Lyons, Paris, and Southern Portugal). Before the end of the year it was again in New York, New Orleans, and the West Indies. Mexico had been visited in the spring, and through the summer.

1854 was still more a cholera year in Europe and in this country. Scarcely any European state or kingdom was exempt. The French, English, and Russian troops suffered from it much in the Crimea. Greece, Italy, Germany, France, Spain, Portugal, in short, all Europe was traversed by it: 150,000 died of it in France alone; in England

---

* London had 13,098 deaths from cholera in 1849; in 1854, about 10,000.

and Wales about 20,000. Newfoundland, on our side of the ocean, was reached for the first time in 1854. This was the year of the epidemic at Columbia, Lancaster County, in this State; so remarkable for the absence of some of the usual promotive conditions of cholera. Our great cities, however, did not suffer nearly so much as in 1849.

In 1855, the disease was widely spread in Europe, though not very malignant except near the seat of war, before Sebastopol. Egypt and Palestine had it also. In Switzerland, which had been slightly touched before, Basle, Geneva, Zurich, and other places now suffered by it. The next year, 1856, still did not witness its withdrawal from Europe.

Since that period, until 1865, I have no means at hand for tracing the movements of epidemic cholera. Dr. Gavin Milroy says that the countries hitherto exempted have been as follows: Australia, New Zealand, and other islands in the Pacific; the Cape of Good Hope and adjoining settlements; the coast of Africa from the Cape as far northward as the Gambia, and including the islands of St. Helena and Ascension; the Azores, Bermuda, Iceland, Faroe islands, and also the Orkney and Shetland; the southern half of the eastern coast of South America, from the Rio Plata inclusive, Cape Horn, and the whole of the western coast of that continent, from the Cape and along the shores of Chili and Peru to Panama.

In last year, every one was familiar with the accounts of cholera in Arabia and Egypt in the spring, at Constantinople in July,* and afterward in several parts of Europe, extending, though with but moderate violence, as far as England. While its vast migrations seem to be as capricious or incalculable as the flight of locusts, two local causes contributed at least to its *severity* in Mecca and on the Nile. These were the crowds of religious pilgrims at the former place, in the spring, and, in Egypt, the insalubrious circumstances attending the operations at the new Suez Canal. In both, "crowd-poison" was intensified to the greatest degree; so that the pest-cause might well find there strength for the renewal of its flight onward to the northwest. In Paris, in 1865, 6383 deaths occurred during the late visitation.

I take from Dr. Brigham's treatise (published in 1832) the following table, of the deaths from cholera in 1832, and their proportion to population:

	Population.	Deaths.	Equal to
Moscow,	350,000	4690	1 in 74
Petersburg,	360,000	4757	1 74
Vienna,	300,000	11,896	1 159
Berlin,	340,000	1401	1 242
Hamburg,	100,000	446	1 224
London,	1,500,000	1223	1 1228

* The first case occurred in that city on the 28th of June.

	Population.	Deaths.	Equal to
Edinburgh,	150,000	72	1 in 2033
Glasgow,	180,000	395	1 455
Hungary,	8,750,000	188,000	1 46
Paris,	800,000	20,000	1 40
Montreal,	25,000	1250	1 20
Quebec,	22,000	1790	1 12
New York,	200,000	2000	1 100
Albany,	24,000	311	1 77

Supposing the population of Philadelphia to have been at that time 150,000, this, with a little over 600 deaths, would give a proportion for our city of 1 in 250 of the inhabitants. In 1849 the ratio was considerably less.*

It is an important fact in the history of cholera, that before, during, and after the epidemic has visited a place, many cases, greatly exceeding in number those of typical cholera, occur, of diarrhœa, sometimes also with vomiting, not violent, yielding easily to treatment. To these the name of *cholerine* is often given.†

---

* Moreau de Jonnes estimates the number *attacked* as, in France, 1 in 300 of the population; Russia, 1 in 20; Austria, 1 in 30; Prussia, 1 in 100; Poland, 1 in 32; Belgium, 1 in 120; Great Britain and Ireland, 1 in 131; Holland, 1 in 144; Germany, 1 in 700.

† The coincidence or anticipation of cholera by epidemic *influenza* and the potato blight, has been several times noticed. But there is, clearly, no uniformity in any such association.

NATURE OF CHOLERA.

Without discussing opinions at length, it may be asserted that cholera is not at all, like our ordinary cholera morbus, a disorder simply of the stomach and bowels. Being clearly an acute *systemic* affection, changes in the blood are *proved* to occur in it, and may well be believed to be primary; that is, that the morbid cause acts through the blood. But that is not all.

Cullen placed cholera, in his nosology, in the class *neuroses*, order *spasmi*. Many medical observers (Binaghi, Loder, Orton, Delpech, Lizars, Coste, Favell, C. W. Bell, Greenhow, G. Johnson, etc.) consider its principal effects to be referable to disturbed innervation, involving chiefly the ganglionic centres of organic life. Dr. Charles D. Meigs, years ago, graphically called the attack the "cholera squeeze." Velpeau, of Paris, lately repeats this, "le mal vous *tortille*." There, I think, is the pathology of cholera, in one word. As Dr. C. W. Bell says, it is not an adynamic, but a dynamic, or sthenic, collapse.

The heart, its left side at least, is, after death, contracted. The pulmonary artery and its branches are narrowed, making the lungs pale and anæmic. The gall-bladder is full of bile, but the duct is spasmodically closed, and detains it there. The

urinary bladder is shrunken to half its size or less. The blood-vessels of the whole alimentary canal press rigidly upon their contained fluid, and force its serum out into the stomach and bowels; whence it is, by spasmodic ejections, thrown out. The very skin is, by its involuntary muscular fibres, as well as by vascular constriction everywhere, drawn tightly and closely upon the body. The voluntary muscles suffer with cramps. All is cramp, cramp, within and without. The brain is almost in anæsthesia during the collapse—no delirium, but apathy—as from cerebral anæmia. The blood, so compressed, grows thick as tar—it scarcely flows, is not aerated, and cyanosis follows;—it is detained in the capillary and venous networks of the interior organs, in which congestion is found after death.

Cholera is, then, I say, a poison-spasm; a ganglionic tetanus.

CAUSATION.

Here we enter, unavoidably, the region of speculation. Not, however, without facts to give us hope and promise. By exclusion, we can first see what the cause of cholera is not.

1. Is it heat, moisture, electricity, ozone, or any modification or combination of the pervading physical forces of nature? Surely not. Those

forces are *cosmic*, not localized in their operation. Observation would hardly be *needed* to show that no state of physical conditions marks the presence of a cholera epidemic; but observation *has* shown it. Sir Henry Holland* compared the atmospheric conditions of five different places in which cholera was prevailing; there was no correspondence at all. Barton has found a high dew-point during cholera in New Orleans; but it prevails often in India where the air is dry. Dr. Moffatt lately has asserted that the equatorial current of moist, ozoniferous air is fatal to the prevalence of cholera. Bérigny, in 1856, thought a deficiency of ozone to coincide with cholera; but Bérigny has, just now, asked and obtained the appointment of a learned commission of the French Academy, to ascertain definitely whether there *is* such a thing as ozone! Dr. Hammond, in Kansas, found no ozonometric reaction during cholera; but Prof. Ellet, of New York, proved, in 1849, that there was no constant relation between the two.† Schultze, Voltolini, and others assert the same conclusion. As to climate, the disease has prevailed from Archangel to the Isle of Bourbon, and from China to Mexico. No *telluric* causation, therefore, can suffice for it.

Elevation has no fixed relation to its occurrence.

---

* Medical Notes, Philadelphia ed., p. 349.

† Hammond on Hygiene, pp. 164–165.

In London, Dr. Farr found the mortality to be directly in proportion to the lowness of site. But cholera has prevailed 9000 feet above the level of the sea, at Bogota, in 1849; 7000 feet, at Emmeneh in Persia, in 1853;* more than once in the City of Mexico, at an elevation of 7990 feet; and in a citadel built upon an isolated rock (Jaragurth, in Bengal),† 1000 feet above the plain. We must explain the influence of lowness of site, as Dr. Baly pointed out, merely as *one* of the circumstances which *modify* the prevalence of cholera; not as accounting for it.

Coming and going, then, across the earth, from time to time, its cause cannot be *conditional* merely. Nor, if it were so, could it happen that, among multitudes of persons, in the same spot, "one should be taken and the others left."

2. If, then, not *dynamic* or cosmic, this cause, though occult, must be *material*. Nor could a *mineral solid*, or *liquid*, possibly so traverse the earth undetected *per se*. It must be, if inorganic, volatile, to cross the ocean alone. That it can do so, has been proved in a number of instances. Those of the emigrant ships, New York and Swanton, in 1848, from Havre, have been already alluded to. In 1854, I had direct cognizance of

---

* Gavin Milroy, op. cit., p. 449.

† Brigham, op. cit., p. 33.

such occurrences, in the packet-ships Tonawanda and Tuscarora, Cope's line, between this city and Liverpool. The first was attacked when two weeks at sea, there being no cholera at Liverpool when she started. After a number of days' prevalence, this vessel neared a large iceberg, which reduced the temperature of the air 30°. The day before the iceberg was met, the largest number of cases occurred; after that, no new one at all. The epidemic was frozen out. In April, 1866, other instances occurred, to which allusion will be made hereafter.

Since, then, I say, the cholera cause can travel far over sea, or land, it must be volatile, or extremely mobile. Is it a *gas?* Chemists have not found such; but that does not quite disprove it. But the *diffusion* of all gases through space, aided by the winds, prevents the local accumulation of gases everywhere, unless constantly emanating from a terrestrial source. Thus the mephitic poison of the celebrated deadly valley of Java, and the carbonic acid stratum of the Cave of Dogs in Italy, are accounted for. No poisonous gas, according to all physical experience, could possibly maintain concentration enough to destroy human life, during weeks and months together, at one place, and then leave it to rest upon another —unless a *source of emanation* were as migratory. As no such mobile *source* is known, the gaseous hypothesis must explode of itself.

3. What *possibilities*, then, are left? I have observed that, *if inorganic*, the "cause" in question must be a gas; but it has been shown, just now, that *no* such gas can have existence; therefore the cause *must be organic* in nature. I see no escape from this dilemma, however wanting we are in physical demonstration of the conclusion it brings on. The *source*, at least, of the cholera-cause *must* be organic.

4. What *organic* sources are *possible* for it? Conceivable theories are—I. That it is a contagious effluvium from the bodies of those sick with the disease. II. That it is generated by a peculiar chemical change in the excreta, either of the sick or the well. III. That it is a cryptogamous vegetation, a microphyte, requiring certain peculiar and only occasional conditions for its existence. IV. That it is minutely animalcular in nature, existing, migrating, and propagating itself only where local circumstances permit.

I. Is cholera contagious? The word is variously defined. I follow La Roche and others in regarding a disease as contagious, only, when its cause is a material produced by a morbid process in the bodies of the sick, and generating the same disease in those whom it reaches, either by contact or at a short distance through the air. In this sense it is proper to assert most emphatically that cholera has never been shown to be contagi-

ous in a single instance; while the always difficult *negative* proof is abundant. Every *apparent* case of contagion (and such are *extremely* rare) is susceptible of a different explanation.

Against contagion we have, especially, the following points:

A. Cholera is often preceded in a place by a considerable increase in the amount of *diarrhœa;* showing the presence of a general atmospheric cause, the intensification or accumulation of which brings on the epidemic.

B. It subsides in a locality, after days, weeks, or months, with no constant reference to the number of persons susceptible to it, but *obviously* in relation to local sanitary conditions.

C. It attacks, simultaneously, places as far apart as London and Newcastle; and often hundreds the same day in a large city. Thus seven thousand perished from it in Paris in eighteen days, in 1832. In the Massachusetts State prison, four within an hour, and two hundred and five within forty-eight hours, were attacked.

D. Cholera is sometimes limited to a part of a town (Oxford, England, according to Drs. Baly and Gull) or even a part of a house; as at Baltimore in the Almshouse, in 1849. Dr. Buckler then reported its existence, when there was no cholera elsewhere in the city, in *one-half* of the Almshouse building, in which *seventeen* fatal cases

occurred; none whatever in the other half, similarly occupied. The *difference* was, that under the walls of the affected side of the house was "a large and foul overflowing cess-pool, whose contents mingled with the washings of the dead-house, etc." In several instances, also, it has been confined to a part of a ship.*

E. An army or other encampment may be affected terribly while in one locality (as with the Marquis of Hastings in India, mentioned before), and then, by a short march, escape from it altogether. Such occurrences are frequent and familiar in India. (See *S. Clark*, Hygiene of the Army in India.)

F. In Hindostan, where cholera is annually present, the facts are of such a nature that the idea of contagion is scarcely entertained by any medical men or others. Dr. James Johnson wrote, many years ago, that "in India, the contagiousness of cholera is denied by ninety-nine out of every hundred medical men." And in a work published in 1864, on the Hygiene of the Army in India (Stewart Clark, M.R.C.S., Inspector-General of Prisons, etc.), I find statements corroborative of this. For example, the following:

"After the Hurdwar and other large fairs in India, cholera almost always appears in the vil-

* *E.g.* steamers England and Virginia, April, 1866, etc.

lages on the lines of roads leading from them; but the cases are generally confined to people who have been at these fairs. For instance, at Deyrah Doon, about forty miles from Hurdwar, there are often two annual visitations of cholera: viz., one in April or May, confined to individuals returning from the Hurdwar fair; and one later in the season, of the usual epidemic type, among the general inhabitants of the town."*

In the words of Dr. A. Flint,† "compare its course as an epidemic, in this respect, with typhus, small-pox, or scarlet fever!"

G. Nurses and others brought into contact with cholera patients, even in hospitals, are not more liable than others to the disease. An official report to the French government stated that in 1831, of over two thousand persons employed in nursing in *hospices* or hospitals, during an epidemic of cholera (which produced in all eighteen thousand deaths), only one hundred and sixty-four were attacked. At St. Petersburg, 1 nurse in 58 had the cholera; in Moscow, of 253 persons connected with one hospital, 4 only were affected by it. In the latter city, also,‡ 587 patients affected with cholera were admitted into a hospital with 860 patients, laboring under other diseases, yet *not a*

* Op. cit., p. 13, note. † Practice of Medicine, p. 425.
‡ Brigham, op. cit., p. 324.

single one of the latter was attacked by cholera. Dr. Alison (himself half a contagionist) recorded the fact* that, in Edinburgh, in 1832 and in 1848–9, the dissecting-rooms were supplied almost exclusively by cholera subjects; and in neither year was there a single case of the disease among the numerous students attending those rooms. Post-mortem examinations have been made freely, by physicians everywhere, during cholera, without any evidence of danger therefrom.

H. No effort to produce cholera by direct contact or otherwise, experimentally, has ever succeeded. Dr. Foy and ten others, at Warsaw,† inoculated themselves with the blood of cholera patients, tasted their dejections, and inhaled their breaths, without receiving the disease. So did the surgeons and medical students at Moscow and Dantzic in 1832; and similar experiments with like results and conclusions were tried by Lizars, Coste, Schmidt, Meyer, Marshall, and others. A late article in the Richmond Medical Journal, by Dr. Houston, mentions that a man in Wheeling, during a cholera epidemic, lay all night in the clothes of another who had just died of the disease; but was unhurt by it.

---

* Brit. and For. Medico-Chirurg. Review, Jan. 1854, p. 22.

† Gazette Médicale, 1831.

Dr. Lauer Lindsay* made elaborate experimentation with dogs; and thought he had succeeded in giving them cholera. But it does not appear so to me, in reading his account. The dogs were exposed in no case less than seven days to the combined effects of confinement, swallowing and breathing the matters of evacuation and perspiration of cholera patients. Then the symptoms ought not at all to be regarded as specific, as the discharges were "decidedly biliary, green, and greenish feculent matter," and "emitting an intolerable stench." Dog No. 1 is also stated to have become *somewhat lively and greatly better*, after having eaten a quantity of the flesh, fat, and blood of dog No 2, which died on the previous day. The expression of Mr. Marshall, another investigator, is clearly correct—that this evidence is still "short of proof" of any contagion belonging to cholera.

I cannot but anticipate that a similar conclusion would follow a scrutiny of the experiments of like character more recently performed by Thiersch in Munich, and Robin at Paris.† They have made dogs *sick* by injecting the excreta of cholera patients into the trachea and veins. No wonder!

---

* Edinburgh Medical and Surgical Journal, April and October, 1854.

† London Lancet, Jan. 13th, 1866.

But, against all this we have the familiar fact that dogs *do not get* the cholera at all by exposure to the companionship of patients, nor by presence during its epidemic prevalence. Every experiment of the kind is simply toxicological, not clinical or *à propos* in any sense.

The influence of *drinking water*, the occasional occurrence of new cases in hospitals to which cholera patients are admitted,* and *all other seeming* instances of contagious transmission, are, as will be shown hereafter, clearly susceptible of *a different explanation*, which embraces *all* the facts.

I. Cholera does not follow regularly, nor depend for its migrations upon, human intercourse. Instances proving this abound in the whole history of it; a few must suffice us here.

Whatever the amount of travel, cholera moves with extreme *slowness*† against the wind. This is especially observable in India; where, as Orton has recorded, it takes sometimes three months to pass over the distance of a ten days' voyage, notwithstanding constant communication.

When the epidemic first reached England, in 1831, after having been in Berlin and Hamburg, it appeared in Sunderland October 26th. *It did*

---

* Brit. Med. Journal, Dec. 9th, 1865.

† Parkes; *vide* Aitken's Practice of Med., vol. i. p. 659.

not reach London until February, 1832; notwithstanding constant communication between that city and the infected district.*

One of the Western Islands, beyond the coast of Scotland, on the other hand, was attacked by the disease, although the intercourse between it and the main land was so rare, that the clergyman of the island continued to pray every week for King William the Fourth, for eighteen months after Queen Victoria had ascended the throne.†

In 1832 and 1848, the town of Annan, nearly equidistant from Carlisle and Dumfries, and right upon the main line of traffic between those towns, escaped cholera altogether, while it prevailed both at Dumfries and Carlisle.‡

I repeat, that these are only instances of a large class of facts, which show a *capriciousness* in the career of cholera epidemics, altogether adverse to the supposition that it is either contagious or in any way dependent for transmission upon human intercourse.

But the advocates of the view just now most prevalent, urge that on all occasions cholera has crossed the ocean only in ships. Each by each of

---

* G. Milroy, Brit. and For. Medico-Chirurg. Review, Oct. 1865, p. 438.

† Aitken, op. cit., p. 650.

‡ Edinburgh Monthly Journal of Med. Sci., April, 1854.

the asserted instances of this has been or could be met and shown to be quite unproven. A *single* case of such disproval would establish for us the important proposition, that cholera certainly does not *depend* for its migrations upon human agencies or contingencies.

What do we say of the *New York* and *Swanton*, vessels, in 1848? They have been already alluded to on a previous page. I say, *first*, that there was no cholera at Havre when they started, nor for some time before; Dr. James Wynne* and Dr. Gavin Milroy† being my authorities for the statement.

Secondly, I regard it as a *decisive* fact, that in all the accounts of the voyage of these vessels referred to, one is said to have been *sixteen* and the other *twenty-seven days out* at sea before any cholera occurred on board.

Now what, according to the strictest contagionists, as well as those who believe in the theory of *excretory fermentation* (Pettenkofer), is the time of *incubation* of the cholera poison? Budd,‡ Niemeyer, and Greifswald make it from six hours to three days; Kiërulf,§ of Bergen, from one to

* Report on Cholera in the United States.

† Brit. and For. Medico-Chirurg. Review, Oct. 1865.

‡ Association Journal, 1854; Memoranda on Asiatic Cholera, 1865.

§ Aitken, Practice of Medicine, vol. ii. p. 657.

four days; Rilliet, Madin, Weissbrod, Hielman, and others, twelve hours to eight days.

How absurd, therefore, to endeavor to construe the case of the *Swanton* as one in which the cholera was *transported* from Havre to New Orleans by that ship! The arrival of the *New York* at the port of that name was not followed by the infection of that city. I would say, that the arrival of the epidemic at New Orleans simply *coincided*, pretty nearly, with that of the *Swanton;* which had been *overtaken by it* on its way. While these pages are being written (April, 1866) two analogous cases are attracting attention; those of the steamers *England* and *Virginia*, from Liverpool; attacked by cholera when five and eight days out.*

J. But something must be said of the authenticated instances where transportation by persons seems to have occurred. With a good deal of research, I have been able to collect together only the following, as allowable in evidence.

Certain cases mentioned by Dr. Jameson, in the Bengal Report, in 1824.

Eight persons taking cholera at Toulon, shortly

---

* Some *conjecture* that German emigrants must have brought the disease to the *England* and *Virginia;* but, if so, why did they not likewise infect Liverpool?

after the arrival there of the frigate *Melpomene*, from Lisbon, with cholera on board, in 1833.* No other cholera cases occurred at Toulon for two years.

A woman attacked at Edinburgh, in 1832, when no cholera prevailed there, after nursing her son, who got the disease at Musselburg and came home sick with it.†

Some instances mentioned by Dr. Simpson (Edin. Med. and Surg. Journal, April, 1838, and Edin. Monthly Med. Journal, 1849), quoted with only partial details by Dr. Alison (Brit. and For. Medico-Chirurg. Review, Jan. 1854).

Three women took cholera after washing the clothes of some sailors who had died of cholera, at Banff, Moray Firth, Scotland, 1832.‡

Two men died of cholera, in 1849, at Campbelltown, Scotland, after the arrival of a woman from Glasgow, with some blankets which had been used by her sister-in-law who died of cholera. The disease then existed nowhere else within fifty miles (Dr. Robertson, Edin. Monthly Journal, August, 1849).

Several instances are given by Dr. Berg, in his

---

* London Medical Times, N. S., vol. iii. p. 515.

† Ten days afterward cholera became epidemic in Edinburgh.

‡ Alison, loc. citat.

Treatise on the Cholera in Sweden, in 1850; of which I have not details.

One case is narrated by Dr. S. H. Dickson, in Charleston, 1832 (Am. Journal of Med. Sciences, vol. xiii. p. 309).

The outbreak of cholera at Arbroath, in Scotland, in 1853, described by Dr. T. Traill (Aitken, op. citat, p. 656). Some instances in the Norwegian Reports of 1850–53. Others in the Report of the College of Physicians of London, 1854. A few cited from a Bengal Report, 1853, by Dr. Aitken.*

A few cases mentioned by Boudin in his "Traité de Géographie et de Statistique Médicales" (tome ii. p. 374), viz.:

Three persons in the commune of Masles, in France, affected, in 1849, subsequently to the arrival of a person who visited Paris while cholera was prevailing there, returned to Masles, became ill, and died of cholera.

One person, in the same year, in the commune of Conde, attacked after handling the clothes of a woman who had died of cholera; there being no other cases at Conde at the time.

Three individuals, in the same year, affected at Aubées, after the arrival there of a person from

---

* Practice, p. 656.

Courville, where cholera prevailed, that person becoming ill and dying of cholera after his arrival.

According to M. Bucquoy.* two wet-nurses, going from Paris to Péronne, and falling ill there with cholera (1865), communicated the disease to eight other persons; some of whom died.

In Dr. Burrall's recent work on Cholera, received since the above was written, several other examples of an analogous kind are given.

All of these, together, would count, I suppose, since 1817, possibly fifty or a hundred individuals, who might be acknowledged to have taken cholera, in immediate sequence upon exposure to contact with the persons or clothing of cholera patients, in localities not at the time under the epidemic influence.

Granted, then, that such was the case. They are, clearly, *exceptional* instances. If cholera was in any proper sense contagious, *could* the *instantiæ crucis* possibly be so few and hard to find or prove? No! But how do we account for these? On the principle of *fomites;* of occasional, very rare, carrying of the cause of cholera, the "germs" of it, in clothing, merchandise, or by the person of a human being; as one might carry skippers on a

---

* Discussions of Parisian Hospital Med. Society; Philadelphia Med. News and Library, Feb. 1866, p. 22.

piece of cheese in his pocket, or a paper of flower-seeds in his carpet-bag.

Practically, what is the difference between this and contagion? Much, indeed! When the cause of the disease is a somatic (*bodily*) contagion, no prevention of it is available, except the total and remote *avoidance* of those *persons* who have it, and of things which have been in contact with them. When the cause is an *extra*-somatic infection, depending, for its production, multiplication, and transportation, on local and atmospheric conditions, not personal—then those conditions may be met preventively; and the very rare carrying power of *fomites* may be reduced to nullity, by sanitary precautions. Against contagion, we would have only quarantine; a most "lame and impotent" defence. Against infection, we have the amply sufficient measures of sanitary police and management.

Contagion, as a theory, would explain only a minority of the facts concerning cholera, and is not required to explain them. Infection will explain all.

Let me here state briefly the main facts in reference to the three vessels which have reached our shores within a few months, having cholera on board.

The *Atalanta* arrived off New York on the 2d of November last, with emigrants, from Havre.

In the steerage there were a number of cases of cholera during the voyage; *none at all in the cabin.*

The steamer *England* reached Halifax April 8th (the present month) from Liverpool, with 1202 passengers, chiefly emigrants, Irish and German. When five days out, she was attacked with cholera. The disease did not then exist, and has not since (as shown by a number of arrivals from that port later) existed at Liverpool. It was, on the voyage and at the quarantine, confined entirely to the steerage.*

The *Virginia* arrived, also from Liverpool, April 18th, at New York quarantine station. She had 1043 passengers, 14 saloon and 1029 steerage, most of the latter being German and Dutch. Leaving Liverpool on the 4th, on the 12th cholera broke out *among the steerage* passengers, in the "orlop," below the deck and beneath the water line: 37 died during the voyage. After reaching quarantine, being detained there, the disease *increased*, but still was exclusively confined to the steerage passengers.

Now, how did these vessels get the cholera?

---

* Eight passengers escaped from the England, and one died of cholera at Halifax; without any other cases following there. Dr. Slayter, of Halifax, went on board of the vessel, took the disease, and died of it.

Not at Liverpool, for there was *none there.* Did German emigrants bring it on board? If they *could* have, why, as they stopped at Liverpool, did they not infest that city also? But yesterday (April 30th), a steamer arrived direct from Hamburg, with no tidings at all of cholera in Germany. Dr. E. B. Dalton, Sanitary Superintendent of New York, and Dr. Elisha Harris, a distinguished sanitarian, went officially on board the *Virginia,* and carefully questioned the passengers in regard to their original starting-places. They had none of them knowledge of any epidemic in any place they came from or through. The nearest possible approach to anything whatever of the kind was said to be that, "*during the past winter,* the disease had appeared in certain towns in Saxony, *near which* some of them had resided!"

Determined, indeed, must any one be to insist upon the *communication* theory, who will account, by a vicinity dating back to "last winter," for an epidemic commencing out at sea, in April, which, yet, in neither of three ships successively so affected (including the *Atalanta* of November), can be or has been communicated *from the steerage to the cabin* of the same vessel! So palpable was the contrast of safety and of infection between the saloon—spacious, clean, and well aired—and the steerage—crowded, close, and unwholesome—that the cabin passengers on one of those vessels pre-

ferred to remain upon it, in their apartments, to being removed elsewhere, in the immediate and insalubrious company of the emigrants among whom the disease had proved so destructive.

These cases appear to me extremely important, as explicable, clearly and consistently, *only* upon the view that the cholera-cause has the power of migration across the sea; a crowded vessel, like a filthy city on land, affording the local conditions for its manifestation, propagation, and extension.

Wishing to give full legitimate effect to all that can be asserted of a different tenor from what I maintain, I quote the following from Dr. Harris, in a recent report:*

"In studying the history of fourteen epidemics of cholera that have occurred within the walls of our New York quarantine establishment, the writer has seen abundant evidence of the infectious agency of the sick and their 'rice-water' evacuations.

"Concerning these repeated outbreaks of cholera at quarantine, it should be stated, that while they proved how fatally infectious the cholera poison may become in the midst of crowded hospitals and public institutions, they have utterly failed

---

* Report on Epidemic Cholera by the Council of Hygiene and Public Health of the Citizens' Association of New York, November, 1866.

to prove that from the same exclusive cause—viz., the contagion of the cholera evacuations—a world-wide epidemic could be caused. These outbreaks did prove, however, that the stools and besmeared clothing of the sick with cholera can, under certain circumstances, propagate the disease; while, on the other hand, a series of events at quarantine and in the city demonstrated, that, for the production of a wide-spread epidemic, other important causes than the presence of the rice-water stools and vomitings must be present."

II. Let us now examine, briefly, the theories which connect the propagation of cholera exclusively with the *discharges* of those who have the disease. They are, that of the late Dr. Snow, of England, and those of Pettenkofer and Thiersch, of Munich.

Snow's theory was, that a poison is generated by the morbid process of the cholera attack; which poison passes from the bowels of the patient; that this poison is conveyed by the water which absorbs it from the tainted atmosphere about him; and thus, through "continuous molecular change," it taints destructively the systems of those who drink such water, or the water of streams polluted by sewage or drainage into which cholera excrements pass.

It has already, I consider, been shown—1st, that there are facts which this theory will not account

for; and, 2dly, that all the facts (many as they are) proving the important influence of drinking water over the mortality of cholera, can be explained as well upon another, wider view.

Pettenkofer's theory is much like Snow's; but he supposes the poison not to pass as such from the bowels of the patient; but that it is produced by a sort of post-excretory fermentation, which requires several days of time, and is effected in the water beneath the surface of the ground. He makes a very large estimate of the importance of the soil and substrata of a place in determining its liability to cholera. He denies that it can ever prevail over rock.

On this I would remark—1st, that it *has* prevailed upon rocky sites;* 2dly, that this theory cannot, any more than Dr. Snow's, explain all the facts; 3dly, that a theory which will account for all other facts, will also explain those on which he relies.

Dr. Parkes and others seem ready to make use of a recent modification of Pettenkofer's views, which is certainly convenient and ingenious. This is, the hypothesis that the "germs" of cholera poison, once produced, are not destroyed for some

* See Brit. and For. Medico-Chir. Review, Jan. 1857, p. 66; also, Brigham on Cholera, p. 33; Drasche, Die Epidemische Cholera, Vienna, 1860; Kiehl, Ueber den Ursprung und die Verhütung der Seuchen, etc., Berlin, 1865.

time by desiccation; that the tainted fæcal matters, though formed in water, may become dry enough to be carried, as dust, through the air.

This is, I believe, at the present moment, the theory which has the greatest number of influential advocates in England; and some elsewhere.

I ask, can it be imagined that such cholera dust (if not endowed with the *vitality* of microphytic sporules or animalcular ova) could *multiply* as the cholera cause must, or could be swept over the ocean far enough to reach a vessel two weeks and more from the shore it had left?

The parenthetical words of this last sentence imply the direction in which my reasonings (and, I hope, those of my reader also) are, by exclusion, impelled.

III. An *organic* nature, that of a living organism, is thus suggested as most probably belonging to this undiscovered cause. The combination of *persistent identity*, with *extensive mobility*, can, I think, be *no otherwise* explained. I regard this as not at all a vague conjecture, the mere resort of ignorance; but as a logically obtained scientific hypothesis; needing but little, if anything, to make it a theory worthy of universal adoption and application. It has been, in such a sense, considered seriously by such careful reasoners as Sir H. Holland, Prof. Alison, and Prof. Max Pettenkofer.

I repeat the question—can it be imagined that

an excrementitious "dust," *not* organic or vitalized at all, should *multiply* itself so as to account for the world-wide spread of cholera?

If it *could*, observe then the other horn of the delemma; how could it ever cease doing so; how could cholera ever stop, until the human race was destroyed? At all events, how could it disappear as it does, after visitation, from Europe and America, for long terms of years?

Variability in reproduction, under complex and partly unrecognized causes, is a characteristic of animal and vegetable life. *This* would account for such facts, and this alone.

The organic theory has been asserted in two forms:* the vegetative or microphytic, and the

---

* *Vide* Holland's Med. Notes and Reflections; Essay on the Hypothesis of Insect Life as a Cause of Disease. I cite from this work the following curious facts:

"The Hessian fly, on its first appearance in America, afforded a singular example of slow progressive movement. First observed in Long Island, in 1776, it proceeded into the interior, at the rate of ten or fifteen miles in the year, destroying all the wheat in its progress. The spread of the *Blatta orientalis* in northern Europe is another fact of a similar kind."

"In October, 1836, a vast swarm of minute *aphides* passed over a wide district in Cheshire, Derbyshire, and other counties. The air was so thickly filled with them, that the clothes and faces of persons walking out of doors were completely covered. From observations taken, the

animalcular. The first is not set aside by any positive facts; but it does not explain any more than the second; while the remarkable mobility or migratory power of the mysterious agent in question points rather to the animalcular theory.

IV. To this, then, we are brought at last; with only one other alternative, as, not a substitute, but an amendment for it. I mean the view advanced recently by two members of the Philadelphia Academy of Natural Sciences, the late Dr. Wilson and J. Cassin;* that reason exists for distinguishing a *third* kingdom of nature, besides animals and plants; for which they propose the name of *Primalia.* If such organisms as sponges, many infusoria, volvocineæ, diatoms, and desmidiæ, have attributes neither animal nor vegetable, and yet clearly organic, it is evident that our previous classifications of natural forms are incomplete.

---

superficial extent of the mass must have been at least twelve miles in one direction by five miles in another; but detached notices from other places make it certain that the continuous swarm was much more widely spread. No proof was obtained as to its rate of movement; but Manchester was infested for two or three successive days. Wherever generated, there is cause to suppose that the swarm was in transit from one place to another, and possibly brought nearer to the earth by some peculiar state of the atmosphere."

* Proceedings of Acad. Nat. Sciences of Philadelphia.

So are our observations. With each increase in their means and facilities, come new discoveries. No limit to microscopic, any more than to telescopic possibilities. Because we have not *found* any peculiar cholera animalcules, or primalia, it does not at all follow that they have no existence.

The chief haunt and residence of cholera being in warm or hot climates, with luxuriance of organic development, this is strongly in favor of the hypothesis. So is its general prevalence in temperate regions during the warmer part of the year.*

Against it, we have two facts especially; as follows:

Firstly, cholera has, on a few occasions, prevailed in cool or *cold* latitudes; as in winter in the north of England, and the north of Russia. As to this, it has been recorded that the winter of 1831–32, when it existed in the north of England, was, as shown by Mr. Losh's Meteorological Journal,† exceptionally mild. In Russia, also, the summer of 1830 was oppressively warm, and the disease began at Moscow in September. In the cities of northern Russia, moreover, the domestic habits of

* September has had, altogether, the greatest number of deaths. *Vide* Brigham on Cholera; G. Milroy, loc. cit., etc.

† Brit. and For. Medico-Chirurg. Review, April, 1856, p. 304.

the population, in heating their houses, maintain a sort of green-house, or internal warm climate, throughout the winter. Thus, any "germs" transported thither in the summer or autumn, if other favoring circumstances were present (as they are), might well be kept alive. Moreover, as Dr. Routh has shown, the poorer Russians throw out everything around their houses; and then melt the snow for their drinking water.

It may be incidentally mentioned, that the meteorological conditions most frequently coinciding with the outbreak and continuance of cholera, are, those of moderately high temperature, and stagnation, or deficiency of atmospheric change or movement.*

Secondly, it has been very plausibly asserted† that no parasite, invading or residing in the human body, ever causes violent or destructive acute disease; but only slow local disorders. Before *trichiniasis* attracted the attention now drawn to it, this statement had more weight than now. Of animal or vegetable parasites there had been, perhaps, before, only one record of any that had endangered life by residence in or upon the body; that of the *mycetoma*, or disease produced by a

* Glaisher, Meterology of London during cholera, etc.; T. H. Greenhow, loc. cit.

† J. Simon, Lectures on General Pathology.

peculiar fungous vegetation attaching itself to the limbs, in India, as described by Dr. Carter, of Bombay. And the effects of this were *slow;* no acute systemic malady was produced. But the lives destroyed by trichinæ lately in some of the German towns afford instances of importance. It is especially notable that these last parasites seem to act by *numbers; a few* may be detected in the flesh of animals or men, without serious injury to either; it is by multitude that they disturb seriously or even kill.

However, the "organic" theory does not require *parasitic* action to make it possible. Life is only predicated as an attribute of the cholera-cause in accounting for its variable multiplication and migrations. Its mode of action upon the human body is a separate question. It is most probable that it is *not* by parasitic residence; but by entering the system as an organic *poisonous material.* For the action of such material, of animal origin, abundance of analogy exists; as in the toxic effects of cantharides, of insect-stings, venom of serpents, and of rabid animals; of dissecting wounds, and spoiled meat or sausage used as food.

My theory, then, is as follows: That the cause of cholera is a (yet undiscovered) protozoon, or primal organism, of extreme individual minuteness; which, on entering the human body, affects it as an organic poison. That the varying quan-

tity or number of these organisms may in different cases account (along with individual predispositions and exposures) for the unequal violence of different epidemics; as in the case of trichiniasis. Choleraic diarrhœa or cholerine, so frequent *before* as well as during and after the prevalence of cholera, may in some instances at least be explained by the action upon the alimentary canal only, of a minimum quantity of the cause. The dreadful fatality of some Indian seasons, is on the same view referred to an extreme *accumulation* of it.

A most important part of the theory is, further, that which concerns *promotive* causation. What conditions favor and maintain in life, multiplication and migration, this *ens primalis?*

All the facts answer, as I believe, that *animal matter in a state of rapid and foul decomposition,* putrefaction, along with moderately high (not the highest) temperature, and ordinary moisture, will afford those conditions; and that *nothing else* is required to explain the whole history of the propagation and extension of cholera. Nothing, I mean, but the admission of the existence of the "protozoon," which in ova or in maturity, or both, may fly "on the wings of the wind;" or be conveyed to less distances by water; and, with these the above-named conditions of its vital maintenance, as its food and "habitation."

It is, in my mind, *obvious* that this theory will

explain all the facts. I believe, also, that *some* well-known facts can be explained by it *alone*.

As to the general facts, I can scarcely do better than repeat, with some extension, words written eleven years ago, and referred to at the beginning of this essay.*

The Gangetic delta being the great focus or centre of cholera, certain traits in the usages and circumstances of that locality and its population have a direct bearing upon the subject. Among these may be named the peculiarity of the inhabitants in disposing of the dead. Most of them either burn the bodies, and this very imperfectly, or throw them into the Ganges or its tributary streams; that river being held to be sacred — a gateway to heaven. In the Hooghly river, Stewart Clark says,† he has seen "upwards of fifty dead human bodies, besides numbers of carcasses of lower animals, floating within sight at one time." At Allahabad, at the junction of the Ganges and the Jumna, many persons drown themselves as devotees, and others are often drowned by the pressure of crowds immersing themselves in the sacred spot.‡ The worship of Juggernauth has

---

* On Animal Decomposition, etc. By H. Hartshorne, M.D. Philadelphia Med. Examiner, August, 1855.

† Hygiene of the Army in India, p. 63.

‡ Stocqueler, Handbook of India, p. 366.

produced a greater mortality still. The average of pilgrims annually visiting the temple of this deity was, a few years since, 120,000. Of these, thousands die from famine, fatigue, and exposure, and are left on the road to rot.

The annual wide overflow of the Ganges, and its withdrawal in the dry season, must increase the amount of animal decomposition. The Nile also inundates, every year, the land of Egypt. How is it, then, that cholera has but half a dozen times invaded the latter country, while always at home in India? I conceive the difference to have a twofold, and yet very simple explanation. The climate of Egypt is proverbially *dry*, from the vicinity of the great deserts. Decomposition there meets with its *minimum* of rapidity. The skin of an Ibis, shot for me in Upper Egypt, simply cleaned without anything antiseptic, and hung up on the deck of a boat in the sunshine, kept for two weeks without any odor or other sign of putrefaction.

Further, the inhabitants of Egypt neither bury nor drown themselves from superstition in the river Nile. If their habits were not, in some localities, especially when crowded away from their homes, uncleanly, there ought, and would, I believe, *never* be any cholera in Egypt.

The Hindoos, however, must be inconceivably filthy in many ways. S. Rogers, F.R.S., a British

army surgeon, gives the following example, in a Report on Cholera in Madras:

"The Coom river nearly encircles the village of Chintandrepett. This river was made a privy of by hundreds of natives, daily; when the water became low, the smell was most offensive. In the hot weather, an attack of cholera was the certain result, the only victims being those residing within a short distance of its banks."

In Europe and the United States, as well as in India, influences belonging to closely aggregated communities have always been observed to display a power to propagate cholera. It comes most often, stays longest, and is most destructive, in the densest and filthiest cities, and in the worst quarters of those cities. In even the densely populated country of Holland, Suermann found the mortality to be 1·54 in 1000 in rural districts; in the towns, 8·93 in 1000. Moscow, Paris, Marseilles, Liverpool, Manchester, Edinburgh, New York, and Quebec have had great mortality from it. That of Moscow, in 1832–33, was 1 death from cholera in 32 of the whole population.

Dr. Baly says, in his "Report" upon cholera in England, that "in the evidence received, lowness of site is not very prominently set forth among the unfavorable sanitary conditions; being, in fact, specifically mentioned only *five* times; while, out of 68 places where cholera raged, bad ventila-

tion and overcrowding of houses are mentioned *fifty-four* times, defective drainage twenty-eight times, cess-pools, open sewers, etc., sixteen times."

Three prisons at Wakefield, all on one plot of ground, seventeen acres in extent, differed* in cholera mortality precisely in proportion to their respective sanitary conditions. Stewart Clark mentions corresponding facts in India. This author also mentions a case narrated by Dr. Thompson, in which cholera affected one side only of a ship, which was foul; the disorder ceasing when purification was effected.

Very important testimony exists as to the influence of the *drinking water* of localities. Having shown that Dr. Snow's theory is insufficient, we find such testimony available still in regard to the propagating and extending power of *animal contamination*. Thus, Bethlehem Hospital, supplied by an artesian well, had, in 1849, among 400 inmates, no case of cholera. It was the *only* large lunatic hospital in London which escaped; as it was the only one supplied with spring water. In the districts of London supplied from the Thames above the entrance of the sewers, the mortality ranged from 8 to 33 in 10,000 of the inhabitants; in those supplied from below the entrance of the sewers, from 28 to 205 in the same number.

---

* Baly, op. cit., p. 20.

In this country, Dr. James Wynne's report* affords, upon almost every page, matter of exactly the same purport as the above. In St. Louis, Louisville, Buffalo, New York, Philadelphia, Boston, etc., similar facts were recorded. It is unnecessary to extract them, they are now so familiar and so commonly accepted.

All of Pettenkofer's and Thiersch's observations, in regard to subsoil accumulation and transit, and fæcal fermentation after discharge, range themselves now naturally under the one general fact which they exemplify, viz., that *animal decomposition is the one great promotive* cause of cholera; to which heat and moisture, etc. are merely adjuncts.

But, that which suggested first to me this opinion was, the singular history of the outbreak at Columbia, Lancaster County, Penn., in September, 1854. Cholera had never visited that town before. It is not large or populous, in a rural site, on the Susquehanna, not densely built enough to exclude malarial fevers. Why should it have cholera at all?

Visiting the town, with other physicians of our city, during the epidemic, I learned that an exceeding drought had reduced the channel of the river to an unusually low ebb, and that, in its bed,

---

* Presented to Parliament, and published in 1852.

a short space above the town, a number of carcasses of sheep and other animals, thrown from the railroad trains, etc., were putrefying rankly in the sun. A reservoir which supplied many of the people with drinking water was filled from the river not far from that spot, and the wind blew from it directly over the town. The first subsidence in the disease, we were afterward told, attended a decided change in the wind.

At Pittsburg, shortly after the above events, a similar epidemic occurred. A gentleman on a visit to that locality not many days before the disease broke out, informed me that the same condition of the river existed there, with a like abundance of accumulated *putrefying animal matter*, exposed to the sun.

In Rhode Island, in the autumn of the same year, I was informed that the local existence of cholera in a few spots, otherwise very healthy, might be traced, in coincidence at least, with a practice not uncommon along the shore of the sea or bays, of dragging up fish in quantities by nets, and spreading them out to rot for manure.

Enough, then, of these facts and of their argument. There remains the great practical question, which they ought to help us to solve—what are the best measures for the local exclusion or prevention of cholera?

PREVENTION.

Quarantine is now urged by some, and appears to be even contemplated by the Government as a part of its duty. Is it available? Will it do any good? I say, *no.* Theoretically, if the views advocated in the preceding pages are correct, it falls to the ground of course. But we have more than that to say against it. It *never has succeeded;* and *never can.* Let us look at the facts.

I take the following from Dr. Brigham's work on cholera, published in 1832:*

"In Russia, immense lines of troops were formed for arresting its progress; St. Petersburg was entirely surrounded by *cordons sanitaires;* but all these regulations, enforced by a powerful despotic government, were unable to prevent the approach and the spread of the cholera throughout the Russian Empire. The efforts of Austria were equally unavailing; for in a short time the disease passed her triple cordons and invaded the country from Poland. Prussia employed sixty thousand of her best troops to enforce her rigorous restrictions, and travellers bear testimony to their severity. And what (says the American Journal of Medical Sciences, May, 1832) have been the results? An immense expenditure of money, the suspension of commerce, a stop put to industry, multitudes de-

* P. 302.

prived of the means of acquiring subsistence, and whole families plunged into misery and rendered favorable subjects for the disease; but *no stop to its extension;* on the contrary, its progress was rendered more fatal. As an instance of this, Breslau may serve as an illustration and a warning to other cities. That city contained 90,000 inhabitants, active, commercial, and industrious, many of them manufacturers and artisans. A quarantine of twenty days, with difficulties almost insurmountable which it entailed, was established at the borders of the province, and maintained with a rigor which might serve as a model to other nations. But, in the midst of this apparent security, a woman living in a damp part of the town was attacked by the cholera, and in a few days the disease spread. *The most minute researches on the part of the public authorities could not discover any communication between this woman and any stranger or goods suspected of being infected.* But when the disease spread, the authorities saw *too late* the deep injury which their sanitary measures had inflicted; a multitude of families, and thousands of individuals, were plunged into extreme misery, for the sudden cessation of commerce, and consequent suspension of labor, had deprived them of the means of subsistence.

"Taught by sad and lamentable experience, Russia, Austria, and Prussia withdrew their cordons, and acknowledged not only their inutility,

but that they are productive of immense evils. Indeed, all the nations of Europe are (1832) abandoning severe quarantine regulations."

Dr. Alison, of Edinburgh, wrote thus in 1854:*

"It is a fact that cholera has made its way, not uniformly, but very generally, *in spite of cordons and quarantine regulations.*"

Dr. Gavin Milroy, one of the ablest and most industrious sanitarians of our time, published, about the same year, an essay with this title: "*The Cholera not to be Arrested by Quarantine.*" A writer in the Brit. and For. Medico-Chirurg. Review (July, 1861) cites from that and many other authentic sources facts of great interest bearing on the subject. This is his account of the state of quarantine in different countries:

"What is most remarkable in the quarantine regulations of different countries at present, is the fact of their want of accordance; hardly any two being alike. Another noticeable point is, that, the more liberal the government of a country generally, and the freer its institutions, the fewer and less stringent are the quarantine restrictions. In the Baltic States, in Sweden, Denmark, Prussia, Holland, the regulations formally enacted may be considered almost as a dead letter; so in Belgium, where they are rather nominally than really in force. In the United States of America, each

* Brit. and Foreign Medico-Chirurg. Review, Jan. 1854.

State of the Union has its own code; all of them, according to a resolution of the Quarantine Convention held at Philadelphia in 1857, inefficient and often prejudicial to the interests of the community. In Chili and Peru, and along the whole western coast of South America, the tendency is to disregard all quarantine regulations, as interfering with the freedom of commerce. In that anarchical country, Mexico, quarantine is under no legislation, the Board of Health having unlimited power, which it sometimes exercises most tyrannically. In the South of Europe, in the old kingdom of the Two Sicilies, the codes are, or were, most elaborate and rigorous. In France and Sardinia, they have of late years undergone revisal; and yet, though somewhat improved, they are still open to great objection; fortunately, however, they are mildly enforced. In the Ottoman dominions—including Egypt, in which, little before 1840, there were no quarantine restrictions—a system has been established as elaborate as could well be contrived, and as inefficient as can well be imagined, being totally in opposition to the feelings and habits of the people."*

All are familiar with the entrance of cholera from Arabia into Egypt, and afterward Europe,

* At Marseilles, in France, the people are said to have lately, under panic, asked for the restoration of the quarantine.

in the spring and summer of 1865. All existing regulations clearly failed then. We cannot but be astonished that, at the late "Cholera Convention," held at the instigation, and almost under the dictation, of the French Emperor, increased rigor of quarantine should have been (against the remonstrance of a number of delegates, it is true) insisted upon as the desideratum hereafter. Whatever validity such an enactment may have in political relations, let us hope that it will have *none* whatever in a scientific aspect; as it contradicts facts, reason, and true expediency.

The *inefficiency* of quarantine is a matter of demonstration. As the author just quoted remarks,* "quarantine, even when rigidly enforced, has not kept out diseases of the contagious nature of which there is no question; such as small-pox, and other exanthemata. In Malta, for instance, we are assured on good authority, that in the short space of seven years, 1829–1835, in spite of quarantine regulations for their exclusion, that island was twice invaded by small-pox, one epidemic proving fatal to 1500 persons out of a population of 114,000; and also by measles, scarlatina, and hooping-cough. Yet at Malta the quarantine system has been enforced very regularly, and under more favorable circumstances as to efficiency than anywhere else."

* Review, p. 40.

Quarantine, if sound in theory even, could not avail, never has availed in practice. Its infraction for smuggling and other inducements, is everywhere constant and notorious; *this cannot be prevented.* Macaulay (History of England, vol. v. p. 52) states that when a contraband trade was, in the time of William III., carried on between France and England on the southeastern coast, "it was a common saying among the inhabitants, that if a gallows were set up every quarter of a mile along the coast, the trade would still go on briskly."

One might think the history of *blockade-running*, during the late rebellion in this country, might afford ample illustration and confirmation of this. Vain, indeed, would be the attempt to close our coast against the introduction of cholera, were it as contagious as small-pox, or as plague was once imagined to be.*

The *evils* of quarantine are great, almost incalculable. Sir John Bowring, speaking in the House of Commons in 1841, gave it as his belief that the losses from quarantine in the Mediterranean alone were not less than two or three millions sterling a year.

But what if, instead of preserving, quarantine

* The Governor of Eupatoria is said to have wished the British and French troops to undergo quarantine, at the opening of the Crimean war!

actually involves, often, sacrifice of *life?* No doubt this has many times occurred. With yellow fever, the quarantine epidemic in New York harbor, a few years ago, exemplified this. In various quarters reports of travellers show the *miseries* and dangers of the lazaretto, and of the confinement on the vessel detained.

What more do we need to show this than the very recent instance of the steamer *England*, at Halifax? Forty passengers, one account says fifty (out of 1202), died on this vessel during the voyage. She was prohibited from entering port; all were detained on board, and, by April 14th, 130 *more deaths* occurred! In all, 159 died while in quarantine. If the twelve hundred passengers had been *landed* and *scattered*, I, for one, doubt the occurrence of the disease in a dozen of their number; especially as it was reported as *altogether confined to the steerage.*

Were such measures sure to preserve from the epidemic the whole people of our continent, a *hecatomb* like this might find excuse. In face of facts, I regard it as a barbarity. Pelissier, in Algiers, was thought a monster, for suffocating a band of guerrillas in a cave; but what is this case of the *England* more like, except in motive? It is closing up hundreds of people for death; as though one might lock the doors, and bar the windows, against all escape of a thousand people from a burning church; such as that of which we

read so harrowing an account, some time back, in South America.

As I write, the papers have account of another steamer, the *Virginia*, with over 1000 passengers, attacked in the same latitude and longitude. A striking confirmation of the view I have advanced.

But it will be said or asked, would you abolish *all* quarantine—abandon all *inspection of ships* whatever? No; I would not. But I would abandon altogether the whole *theory* of quarantine, as against cholera most particularly.

Ships should be inspected on approaching ports, because they may have unsanitary conditions intensified in them, on a scale sufficiently large to be important. This is, or should be, a part of sanitary police. Nor should it (and here is a great point of difference) include *any* restriction of *persons;* at the most, longer than enough for cleansing of the body and of the clothing, and purification of merchandise, by fresh air, and possibly by some disinfecting process in certain cases.

I insist that SANITARY POLICE includes the sum total of available measures for the prevention of cholera in any place.

On this ground, the measures required are obvious, and familiar. The thorough and frequent cleansing of all streets, alleys, courts, wharves, and vessels, private and public buildings, and empty lots; the abatement of all nuisances; daily

removal of offal; efficient sewerage; and *conservancy, i.e.* the cleansing, ventilation, and disinfection of cess-pools and water-closets. Among all signs of danger of the location of cholera, none is more significant than the *privy odor*. Let it be everywhere annihilated. Lime, charcoal, dry earth, chloride of lime, Labarraque's chloride of soda, liquid coal tar, chloride of zinc, and sulphate of iron are about the most available of disinfectants.

The fresh white-washing of cellars is useful; thorough ventilation and drying of them and of all parts of habitations, still more so. Chloride of lime may be placed, in a saucer, in any suspected room or other locality in a house. The same, in the solid form, or solution of green vitriol, may be thrown daily into a foul privy; and, during cholera time, especially in the case of patients with the disease, every water-closet and vessel used may and should be disinfected constantly, by a dilute solution of chloride of zinc, chloride of soda, permanganate of potassa, or carbolic acid. The immediate removal of all discharges from the sick-room, their disinfection and transportation to the safest possible place of elimination, ought to be imperatively maintained. All foul clothing must be promptly washed, or, if very bad, disinfected or burned.

These precautions have been proved to be capable of essentially limiting and mitigating the prevalence of epidemics.

PERSONAL PREVENTION.

One principle will suffice here: *to keep the system at par;* neither above its level of excitement, nor below that of its due strength.

For this, regularity of life is required, in work, diet, mental movements, and all indulgences. The popular errors most common are, one, to suppose that living on rice or rice-water, avoiding fruits or vegetables, etc., will be preventive; another, to think constant alcoholic stimulation beneficial for that end. *Both are certainly wrong.*

In 1832 and 1849, the late Dr. Joseph Hartshorne, my father, then in very large practice, allowed in his family all its usual variety of food: boiled corn, peaches, watermelons, cantelopes, etc., everything but cucumbers; and no cholera resulted from the liberty. My own subsequent experience justifies the practice. Of course care is always needed as to *quality* and *quantity.*

Of all those most likely to die when attacked by this disease, the drunkard stands first, according to all records. Nor is he one whit less apt to be attacked than others. Temperance, *in all things,* is essential to safety during epidemics of every kind.

TREATMENT.

To *discuss* all the modes of management proposed for cholera, would make a volume larger than this is meant to be. I shall merely *enumerate* those which have attracted the most attention; and then give my view as to what is so well sustained as to be worthy of further trial and some confidence.

1. Bleeding.—This was largely practiced in India, in 1818–1825, by Corbyn, Scot, Annesley, and others. Without entering upon any argument about it, I will simply say, that (as Dr. Brigham's quotations show) as many positive facts have been asserted on behalf of the success of blood-letting as of any other remedy in cholera. My father bled in several cases in 1832, and had confidence in the treatment, as "the most effectually *antispasmodic.*" In 1849 I bled in one case (a boy of twelve years of age), in incipient collapse. The blood at first was thick and black as tar; in a few minutes it flowed more freely, and the patient *recovered.* I confess that the only thing which makes it unlikely that I will ever try or advise the repetition of this practice is, the want of courage to stem the overwhelming tide of professional and popular opposition now existing against it. In this timidity I may be wrong; if

so, another generation may afford the demonstration of what is right, in such a way that no one can gainsay it.

2. Calomel.—This, too, was an old East Indian remedy. Suggested by the almost universal absence of bile in the discharges, which was thought to indicate the need of stimulation of the torpid liver, it has been more largely given than any other medicine in cholera.

Unhesitatingly, I hold the opinion that calomel is of no earthly use in cholera. The argument in its favor, from the absence of bile in the stools, is rebutted by the fact of its abundance in the gall-bladder; while the clinical experience quoted for its success is accounted for by the addition to it, almost always, of opium, in the prescription. Nor is the amount of success with it, even then, great. Such is Dr. Gull's conclusion, based upon the examination of a great mass of evidence, given in his report.*

Dr. Ayre, a British practitioner of some note, gave prominence to a modification of the old calomel treatment (in which twenty grains were sometimes given at once), by prescribing a grain of calomel every five minutes during the attack.

3. Saline Treatment.—Dr. Stevens, of Jamaica, proposed this, upon the view that the main patho-

---

*Report, etc. of Drs. Baly and Gull, already cited.

logical element in cholera was the loss of salts from the blood in the discharges. After the general failure of saline solutions (of common salt, carbonate and phosphate of soda, etc.), given by the mouth, had been conceded, Dr. Mackintosh, of Edinburgh, and others, tried the method of injection into the veins (half an ounce of common salt, and four scruples of sesquicarbonate of soda, dissolved in ten pints of water, at 105° to 120° Fahrenheit). Under this plan, resorted to during collapse, of 156 patients in Dr. Mackintosh's hands, only twenty-five recovered. Remarkable improvement, almost like a resurrection, appeared in several, who afterward fell again into collapse, and died. The suggestion has been recently made, that it may have been the *temperature* of the injected liquid which produced the benefit, so promising and yet transient.

4. Eliminative Treatment.—Dr. George Johnson, of London, has urged this with especial vigor. The castor-oil medication of cholera owes its trial to him. Some recent lectures of his on the pathology and treatment of the disorder give a full and very intelligent exposition of his views. A prominent idea with him is, that the general collapse is due especially to anæmia of the lungs, owing to spasmodic contraction of the pulmonary artery and its branches. I regard this as only a *part* of the *universal* arterial (and other) involun-

tary muscular spasm, belonging to what I have called the *ganglionic tetanus* of the collapse. But the essential feature of Dr. Johnson's pathology is the opinion that, the disease being toxæmic, a morbid poison exists which must be *eliminated* from the blood; and that the discharges are the media of this elimination. Therefore, the vomiting and diarrhœa are salutary or relieving; and ought to be rather encouraged than checked. He goes even so far as to repudiate the commonly accepted belief, that "premonitory diarrhœa" or "cholerine" ought to be checked; considering it a fallacy to assert that those who are relieved of such symptoms by mild treatment were really, or would have been, cases of cholera at all.

I am entirely unable, from observation or reflection, to assent to these views. They have very few advocates or supporters, besides the distinguished physician whose name and ability command for them at present careful consideration. It is true that patients have died of cholera without vomiting or purging. I saw in 1849 a woman in collapse (from which she recovered) for several hours without either; and many such cases are on record; though, in some, after death, the intestines have been found to be distended with the rice-water liquid. But the checking of the discharges is almost always the sign of the improvement and recovery of the patient. And we

cannot, on Dr. Johnson's dictum, set aside or quash all the accumulated evidence, in Europe and in this country,* which shows that it *is* desirable and important to *check all watery diarrhœas in cholera time*—such fluxes having been proved to be often premonitory of cholera attacks.

5. Ice to the Spine.—Dr. John Chapman's ice-bags threaten to become the "pathy" or therapy of the day, with those who are zealous and venturesome in experimental practice. Upon reasons of a physiological nature, not appropriate for discussion here, I disbelieve altogether in the theory of his therapeutics. In his pamphlet upon "Diarrhœa and Cholera," lately published, he gives but one case of the latter disease, and does not say whether the patient recovered or died.

As ice is so useful when internally given in cholera, it *may* be safe and beneficial when applied to the spine. Not having seen it tried, I am not prepared to deny the possibility. It is one of the experiments to consider, in so desperate a disease. But, if it *should* hereafter prove useful, I should explain that result quite otherwise than Dr. Chapman has done, in part at least.

6. Sulphuric Acid.—Dr. Cox, of England, after-

---

* See Lectures on Cholera, by Prof. A. Clark, of New York; Report to the Royal College of Physicians, 1854; also, Madin, Briquet and Mignot, etc.

ward Mr. Buxton and Dr. Fuller, and very recently Dr. Jules Worms, of Paris, have especially recommended dilute sulphuric acid in all stages of cholera. Many others especially report well of its action in the *premonitory diarrhœa*. Such an action would comport perfectly with the view I have taken of the *organic* nature of the poison of cholera; sulphuric acid being so potent a destroyer of everything organic, except such *mirabilia* as the Acarus Crossii.

Dr. Worms' treatment (based on the results in 238 cases of cholera, and 150 of cholerine, in 1865) is as follows: For prodromic diarrhœa, he makes a "mineral lemonade," of about half a drachm of concentrated sulphuric acid to a pint or more of sweetened decoction of salep (arrow-root would do as well). The patient is to take of this every hour a wineglassful, till relieved.

For confirmed cholera, the patient being kept in complete repose, there is administered every half hour a glass of a similar lemonade, of the strength (about) of a drachm to the pint; ice and wine also being allowed *ad libitum*.

7. Opium in large doses.— This practice had once many advocates; now they are few. Prof. Austin Flint, of New York, is one of them; at least *morphia* is advised by him, in full dose, repeated if required. A great deal of evidence of the insufficiency of such a plan has been published;

although it is *not worse* than several other methods. Letting alone would probably be better. The *secondary fever* is apt to be more severe and more often fatal after treatment of the attack by large doses either of opiates or stimulants. Large quantities of brandy (I add, by the way) have been often used, with no good results.

Statistics are given, as follows, of the results of some of the most common modes of practice in cholera, by practitioners in Great Britain, as reported to the "Treatment Committee of the Medical Council of the Board of Health," 1854–55.

Taking all grades of the disease, the deaths were—

	Per Cent.
With Eliminants	71·7
Stimulants	54·0
Calomel and Opium	36·2
Chalk and Opium	20·3

Of *collapsed* cases, the mortality was—

	Per Cent.
With Calomel and Opium	59·2
Larger doses of Calomel	60·9
Salines	62·9
Chalk and Opium	63·2
Calomel, small doses	73·9
Castor Oil	77·6
Sulphuric Acid	78·9

Much is uncertain, obviously, in such statistics, without further account of dosage, circumstances,

etc. But this seems to follow; that *neither treatment has much to boast of success.*

8. Treatment by antispasmodics and mild stimulants, in small doses at short intervals; with ice, and external frictions, etc.—In 1849, my first two cases of cholera were fatal; although assiduously watched, each for a day and a night. The third, I saw with the late Dr. Wm. E. Horner, Professor of Anatomy in the University of Pennsylvania. I left the treatment to him. He sat down by the bedside of the patient—a man, blue, cold, and with a scarcely perceptible pulse, copiously vomited and purged, with rice-water. Having ordered ice,* Dr. Horner took from his pocket a vial containing a mixture of chloroform, oil of camphor, and laudanum; which he gave in sweetened ice-water, in small doses, *every five minutes* by the watch. Each dose was followed by a piece of ice.

Soon the vomiting diminished, afterward the diarrhœa, and in an hour and a half the veins on the back of the hand began to fill up, and the blood to return in them more rapidly after pressure. Diminishing the frequency of the doses, we left him, an hour later, evidently convalescent. When I saw him after several hours again, he was

---

* Ice was used, and lauded, in cholera, by the celebrated Broussais, in 1832.

sitting up in bed, at ease, and so changed, that I doubted at first his identity. No secondary fever followed; he was cured.

Naturally, I repeated this treatment in all my subsequent cases, some of which were of extreme severity; and with gratifying success. The memorandum-book of the number of these cases has, to my present regret, been mislaid. After the treatment of Prof. Horner had been adopted, however, I saw no death, except in the instance of a drunkard, two or three hours in collapse before any medical treatment began.

Should I be attacked with cholera, such is the treatment I desire. Conscientiously I believe, that nothing else will afford a better chance of recovery. I merely altered Prof. Horner's mixture to a tincture, for better preservation; adding some minor adjuvants. This recipe will be given directly. Frictions and sinapisms may also be added. The great merits of this plan are its antispasmodic nature, and the administering of small doses at *very short intervals*. This is eminently demanded in cholera. Phthisis may be a complaint of years; hooping-cough, of months; typhus, of weeks; pneumonia, of days; but cholera must be numbered by its hours, half hours, or even minutes.

Having reached, then, this conclusion, I may

add, that a *rationale* for such a treatment is discernible. I only follow many good authorities in the opinion that cholera is, symptomatically and pathologically, a poison-spasm, or tetanus of the ganglionic system. Taken early, that condition may be *prevented,* by mild opiates and stimulants, in the *premonitory* stage. Later, while any medicines will act, these will do the most. What is needed in confirmation of this explanation, more, than is given by the action of quinine in preventing an anticipated chill, or, of the same, in full quininization, curing the paroxysmal disease (a toxæmic *neurosis*) of intermittent? An antagonistic influence against that which so perturbs innervation throughout the body; such is the whole definition that we can give of the remedial power shown in either case.

Let me be more specific in reference to treatment. Premonitory diarrhœa is very generally admitted to be present in a majority of cases of cholera.* In the East Indies, many writers, of different dates (Lawrie, 1832, Stewart Clark, 1864, etc.), assert such a stage to be an exception instead of the rule. But, in India, they have a premonitory or incipient stage of another kind;

* Barraut asserts fixed contraction of the pupil to be the first prodromic sign; M. Worms makes the same statement in regard to albuminuria.

characterized by great languor or depression, with restlessness, and sometimes ringing in the ears, occurring mostly in the night. Stewart Clark states* that, in this stage, a mild opiate ("with a little calomel or blue pill"), with a cup of warm tea or a small dose of a diffusible stimulant, as a few grains of carbonate of ammonia, or a little weak warm brandy and water, will arrest the attack in a great portion of cases otherwise to become serious.

Such symptoms, as well as diarrhœa, should be noticed here, during a cholera epidemic; and I believe the same treatment will meet both. Rest, warmth, and mild, composing, but gently stimulating draughts; paregoric, aromatic spirit of ammonia, tincture of ginger, lavender, etc., with a mustard-plaster over the abdomen, and a hot mustard foot-bath if coldness of the body increase, or vomiting begin; such are safe, and I believe will be efficient remedies. The above may be called the first or prodromic stage.†

The next has been well called, by Prof. A. Clark, the *rice-water* stage. For that, the treatment I have described as given to me by Prof.

---

* Hygiene of the Army in India, p. 12.

† The recently published experience of Dr. Hamlin, in Constantinople, confirms the importance of the above early treatment.

Horner is particularly adapted. My recipe, based upon his, is as follows:

℞.—Chloroform. et
Tinct. Opii et
Sp. Camph. et
Sp. Ammon. Aromat. āā f℥jss;
Creasot. gtt. iij;
Ol. Cinnamom. gtt. viij;
Sp. Vin. Gall. f℥ij.—M.

Dissolve a teaspoonful of this in a wineglassful of ice-water; and give of that two teaspoonfuls *every five minutes;* followed each time by a lump of ice.*

Friction of the limbs with brandy and red pepper will be, along with large mustard-plasters on the back and pit of the stomach, useful to promote reaction.

The third stage is that of absolute collapse; blue, pulseless, shrunken, voiceless. Should a case go on, in spite of the above-mentioned treatment, into this state, what else can be done? All now

* I take from Dr. Aitken's Practice the following recipe, much used and approved in India and England: ℞.—Ol. Anisi, Ol. Cajeput., Ol. Juniperi, āā ℥ss; Æther, ℥ss; Liq. Acid. Halleri (*i. e.* one part concentrated sulphuric acid to three parts of rectified spirit), ℥ss; Tinct. Cinnam., ℥ij.—M. Dose, 10 drops every ½ of an hour, in a tablespoonful of water.

seems to be desperate experimentation.* Let the ice-bags be *tried*, and judge them by the trial. I would also try belladonna internally, as an antagonist of vascular spasm. Leclerc, of Tours, introduced it in 1854; Barraut, of Mauritius, used it ($\frac{1}{4}$ grain every half hour), and reported success. He also employed *hypodermic injections of sulphate of atropia.* This should be tried again in bad cases. So might be, as was suggested by me in 1855, *warm baths of infusion of stramonium* (Jamestown weed) *leaves;* on the same indication. Also, the injection of hot liquids into the rectum; the warm bath (hot baths cause *distress* in the collapse), with *carbonate of ammonia* added, as used sometimes in malignant scarlet fever (West) in children; or, the warm *mustard* bath. *Hot air*† bathing, if practicable, in the manner so praised of late by Erasmus Wilson and others, would be worth trying; and so would even the inhalation of nitrous oxide. Let us confess honestly, for it is wise to do so, our art is here very weak; *fifty per cent. or more of collapsed cases die;* shall we not endeavor to *discover* new

---

* Duchaussoy and Vernois assert the non-absorption of medicines given by the stomach during the collapse; but Magendie proved that a very slow absorption does occur.

† Dr. George Johnson states that he has seen the hot-air bath used without success.

resources? All honor to those who, at the risk of their own lives, contend yet, with so forlorn a hope, and so little glory to be won. There is room yet for, and possibility of obtaining, a final triumph.

Two words remain still to be said, with short comment: *house to house visitation*, and *houses of refuge*. These are measures of great consequence, shown to be of value during cholera epidemics. The latter, especially, is of notable importance; that is, the establishment of houses of refuge in *salubrious places*, into which persons from tainted districts most liable to the disease may be received, on the occurrence there of the first cases.

That there *are* such tainted districts, has been amply proven. Thus, Dr. Laycock has shown that in York, England, the first death from cholera occurred in the spot where plague had been traditionally the worst, in a badly-drained district. In Edinburgh, the first case in 1848 occurred in the same house as did the first in 1832. In Holland, at the town of Groningen, in 1832 and 1848 but two houses in the better part of the town were attacked; the same houses exactly in both epidemics.

Dr. Alison reports that in the first three months of the epidemic at Edinburgh, in 1832, 353 persons were taken in at Houses of Refuge, from 70

tainted districts, houses, and rooms in which decided cases or deaths had occurred. Of these, only 15 took the disease, and 7 died after removal. Of the 346 thus surviving brief exposure, it is very probable that more than half would have died had they remained in the midst of the infection. At Glasgow, in 1849, 401 persons were taken into Houses of Refuge from tainted districts; only 19 of these took the disease and but 5 died. At Oxford, England, the same year, of 70 persons so taken in, none died. The London Board of Health, in its "General Report," gives the fact that of 1691 of whom the Board had accounts as taken into Houses of Refuge, but 33 were attacked, with only 10 deaths. These numbers would have been larger, but for the very common unwillingness of poor and ignorant people to leave their homes, chiefly from want of confidence in the greater safety of so doing. Could this be overcome, I have no doubt that an immense saving of life might be produced by Houses of Refuge, allowing also the places which are proved "foci of infection" to be thoroughly purified at once.

House to house visitation, by sanitary inspectors to abate nuisances, small and great, and by medical men to treat premonitory symptoms, might also have great preventive value. The establishment of cholera hospitals may be made necessary when the number of cases is great, espe-

cially as the greatest proportion always happens among the poor, who are ill provided for attendance at their homes.

There should be no *panic* about cholera; especially in Philadelphia. Let our authorities do their full duty, in sanitary measures of local improvement and renovation; let them give to our city its pristine cleanliness, and cholera may reach and sweep across the continent without a case occurring here. Baltimore has had such an immunity more than once. And, in our worst visitation, the mortality has not been very great. Fear, moreover, aggravates the danger.

While, if it come, those who can leave as well as not, will, undoubtedly, be most safe in the open country, for those whose duties keep them in the city, courage and equanimity are not only becoming but expedient.

There is but slight exaggeration in the proverbial assertion, that "Pestilence kills thousands, but Fear tens of thousands." Above all, let us hope that no mistaken terror of contagion will ever lead to the extreme barbarity of desertion of the sick or neglect of the dead.

Cholera is not, after all, a hard death to die. To me, it appears one of the easiest modes of exit from the world.